SELFRIDGE (J.M.)

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RELATION TO HEALTH AND DISEASE

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-BY-

J. M. SELFRIDGE, M. D.

OAKLAND, CAL.

Chairman of the Bureau of Anatomy and Physiology

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THE ANATOMY AND PHYSIOLOGY OF BACTERIA

AND THEIR RELATION TO HEALTH AND DISEASE.

It is with considerable diffidence that I approach a subject to which some of the ablest scientists of the age have devoted the energies of a lifetime in the investigation of its problems, some of which remain unsolved.

The literature of bacteria is so voluminous that it will be impossible, in a paper like this, to give a complete résumé of the published investigations. Neither is it expected that anything new in the *anatomy* or *physiology* of these minute organisms will be mentioned, but my hope is that the truths already established may be so presented as to demonstrate, if possible, the true relation that the bacteria sustain to *health* and *disease*.

When we consider that the bacteria "are the smallest of all microscopic beings; that some of them are situated at the extreme limit of our highest magnifying powers, and that their proportions, as to length and thickness, are comprised within the limits of errors of observation," we may, perhaps, have some idea of the difficulty of determining their exact anatomical structure. Think, for example, of studying the skeleton of a micrococcus, whose diameter is only the one-fifty thousandth of an inch. The minuteness of such an object cannot be comprehended, but may possibly be imagined when we compare it with other objects, e. g.: Take a ball one inch in diameter and decrease it fifty diameters. We will then have an object equal to one of the periods used in punctuating printed books, which is about the one fiftieth of an inch in diameter. Now, if we decrease this minute ball ten thousand diameters, we will then have a globule the

dimensions of which will be equal to a micrococcus. Again, take the one inch ball and *increase* it fifty thousand diameters, the size of such an object, by actual computation, will be a globe *one mile* in diameter.

The skeletons of the bacteria are simple structures, having only thin cell walls, so delicate that their very existence has been questioned by such observers as Warming, who contends that their appearance "may be the result of a peculiar condition of the plasma, which, in all the bacteria, is of a more consistent nature than in other plants." The researches of Hoffman, however, have shown that they have a true cellular structure. Cohn, also, claims to have succeeded, with high powers, in seeing directly the cell-membrane.

Not only so, but chemistry has afforded the most positive proof that they have an envelope of cellulose, which is colored by tincture of iodine; is not destroyed by caustic potash, ammonia, or even acids, and resists putrefaction for an exceedingly long time. In this respect it resembles the membrane of cellulose of vegetable cells.

The anatomy of the bacteria, as we have seen, is not in the least complicated. It consists of a small quantity of protoplasm inclosed in a delicate membrane constituting a very minute cell. These little cells are called by different names, which correspond to their different forms. Some of them are armed with *cilia* and are endowed with considerable locomotive powers.

At this point a pertinent question presents itself, viz: What place in the scale of beings do these little organisms occupy? Are they animal, or are they vegetable?

This question has been a source of much controversy among scientific observers, and for a long time remained undetermined.

Movement, at one time, was supposed to belong exclusively to animals, but it was found that it also belonged to a certain number of inferior vegetables, a discovery that mystified rather than answered the question.

There are some observers of high repute, like Häckle for example, who have created for these minute beings

such as the monera, protoplasts, flagellata, diatoms, etc., an intermediary kingdom between the animal and vegetable, and called it the Protista. This, however, is not generally accepted.

This question was finally settled by chemistry. It was found that a concentrated preparation of liquor ammonia dissolves the eggs, the embryos of all animals, the bodies of all the inferior infusoria, attacks the spermatozoa, etc., while it leaves absolutely intact all varieties of cellulose and the anatomical reproductive elements of plants, whether used hot or cold. Results as positive in other directions have been observed in experiments with concentrated acetic acid, iodine and sulphuric acid. Now, since the same results have been observed when treating these organisms with the chemicals mentioned, it follows, therefore, that no one ought to give to the bacteria, as do some recent authors, the names of "microscopic animalcules." for whatever differences of opinion there may be among naturalists as to the place of the bacteria among the cryptogams, there is but one opinion as to their nature,—*they are vegetable.*

Their growth is very simple, and when watched under the microscope is exceedingly interesting. When all the conditions are favorable, the rapidity with which they multiply is simply marvelous. The manner in which it is done is usually by fission, which "consists in a transverse division of the cell," or, to speak more scientifically, when a bacterium is about to reproduce itself "the protoplasm becomes clearer in the central portion, and a partition forms in the median line, separating the contained protoplasm into two portions." The conditions which favor this process are a certain degree of temperature and a sufficient quantity of nutritive material. The temperature best suited to their multiplication is from 90° to 100° Fahr., but if it be depressed so as to approach the freezing point, the process is completely arrested. On the other hand, if the temperature be raised to 130° or 140° Fahr., they are destroyed. But the spores, which is one of the methods of propagation, will resist a temperature of 212° Fahr.

As has already been said, the multiplication of bacteria is very rapid, so rapid, that if nothing occurs to interfere with the most favorable conditions, the number of these little beings that will invade a culture fluid, in a given time, can only be arrived at by calculation. "Let us suppose," says Cohn, "that a bacterium divides into two in the space of an hour; then into four at the end of a second hour, then into eight at the end of three hours; in twenty-four hours the number will already amount to more than sixteen millions and a half; at the end of two days this bacterium will have multiplied to the incredible number of 281,500,000,000; at the end of three days it will have furnished forty-seven trillions, and at the end of a week, a number which can only be represented by fifty-one figures." Now, if we admit that "the capacity of the ocean will be 928,000,000 of cubic miles, the multiplication being continued with the same conditions, the bacteria issuing from a single germ would fill the ocean in five days."

When speaking of the reproduction of bacteria, Magnin says: "they must receive nourishment and respire in the same manner as all the colorless vegetables and as all inferior animals deprived of special apparatus—that is to say, by endosmotic absorption." Again, he remarks: "Although the media in which the bacteria develop are various, yet, from the point of view of the nutritive function, they act everywhere according to the same laws." And again, "No matter in what medium they live, they must have water, nitrogen, carbon and oxygen, as well as certain mineral salts which enter, but in quantities exceedingly minute, into the chemical constitution of all organized beings."

According to the observations of some scientists, the appearance of the bacteria is changed by the kind of material upon which they feed. For example, Hallier, Trécul and others, have observed "the transformation of bacteria into 'levûres' (the yeast fungi), and these into Penicillium." On this point, H. G. Schneider remarks, "They can like the Penicillium glaucum,* thrive upon the most varying soil, but the form of their vegetation is changed thereby so extraor-

* Blue mold.

dinarily, that, for this very reason, the various forms were described as different species, each of them requiring a different maternal soil." "The experimental transformation of the harmless hay-bacillus (B. Subtilis) into the deadly bacillus anthracis has been claimed by Buchner and Nägeli; and Prof. Greenfield claims to have transformed, by a series of culture experiments, the anthrax-bacillus into a harmless form not distinguishable from the hay-bacillus." Although Koch thinks "that these are distinct species," yet it is a fact "beyond question that the anthrax-bacillus may undergo a remarkable modification as regards virulence."* That this is so, ought not to excite surprise, for the same thing has been observed in beings of a higher order. Those who have given scientific attention to breeding, know what transforming effects food, etc., have upon animals. The results obtained in this direction by the experiments of Darwin are quite remarkable, so much so that he claims it as a *law* of nature.

To those persons who have given no thought to the subject, the general diffusibility of bacteria is one of its remarkable features. Upon this point Magnin remarks: "The bacteria are of all beings the most widely diffused. We meet them everywhere—in the air, in water, upon the surface of solid bodies, and in the interior of plants and animals."

But whence do they come? This is a question that naturally presents itself, but is not so easily answered. The scientific infidel—like Hackle, for example—is only too eager to claim that they "are produced by heterogenesis; that is to say, by creation outright from mineral or organic substances." In other words spontaneous generation. But according to others, "they come directly from individuals like themselves, by one of the known modes of generation, viz: fission, spores, etc. Finally, it is believed that they are derived from organisms already existing, and are nothing more than different states or phases of development of known species, of which the life cycle is not yet discovered." This latter hypothesis constitutes what is

* Sternberg.

called polymorphism. Neither of these hypotheses seem to properly answer the question of their origin. The first is not only unreasonable but untenable, having been completely refuted by the ingenious experiments of Pasteur. The second is no answer at all; and the third would not account for certain conditions closely related to this part of the subject. For example, it is not surprising that bacteria are found on the surface of the human body, and in the interior of organs in communication with the exterior; but to account for their presence in the interior of organs, having *no* external connection, is one of the conditions this latter hypothesis does not explain. Magnin seems to think that such a condition forces us into the "presence of two hypotheses: one admitting the spontaneous production of these organisms in the interior of the tissues," and the other explaining it by the "introduction through the membranes of the germs of bacteria from without." Cohn, I know, has shown that the passage of the germs of bacteria are not "arrested by the superposition of sixteen filters." Still the latter hypothesis seems too absurd to be seriously considered, for it seems impossible that the bacteria can force themselves through the tissues where there is so much outward pressure as there is, for example, in acute abcesses, the pus of which, according to Bergron, Billroth, Cheyne, Agston and others, swarms with bacteria. Bergron says he "cannot admit that in these cases the vibrios have penetrated into the interior of the abcesses through the lymphatic system, or through the circulating system, both being intact." Bergron could, with propriety, have added, that tissues, able to resist the outward pressure of contained *gases*, would certainly *not* permit the entrance of organisms from without. It is clear, therefore, that the presence of bacteria in abcesses remains to be accounted for.

The hypothesis which admits the spontaneous production of these organisms in the tissues, if modified somewhat, would answer the question, *whence come they?*

The bacteria, in accordance with a *provision of nature*, are the result of the *decomposition of organic matter*. This hypothesis will account for their presence wherever found.

Decomposition is as much a law of nature as is nutrition.

The transforming processes of decomposition always proceed with a rapidity just in proportion to the cessation of vitality. When an organism is to be nourished, certain kinds of pabulum, suited to its peculiar necessities, being furnished, are transformed, by a process (none the less wonderful because it is common) from inert matter into the vitalized cell of the vegetable and the living blood corpuscle of the animal. Although we know that this is done in obedience to the laws of nature, yet none of us have witnessed the process, threfore cannot describe it, and are only permitted to announce the result. So, by an *inversion* of the *laws* of *vitalized chemistry*, the dying or dead organism is *transformed* into gases, bacteria and inert matter.

That bacteria should be the result of this disintegrating process is as *reasonable* and certainly no more *wonderful* than the process of digestion and assimilation. Not only is this so, but bacteria will not thrive nor multiply where the conditions necessary to the production of decomposition are not present. First of all, the vitality of the organism must be depressed or entirely destroyed; and, in addition to this, a certain temperature is *absolutely essential*. For example, milk will not sour nor the yeast fungi propagate if the temperature be depressed to 32° or raised to 140 Fahr., but they will thrive *admirably* in a temperature of 90° to 100° Fahr.

But, it may be asked: Do not the experiments of Pasteur, which so completely refuted the theories of the heterogenists, apply with equal force to the theory that bacteria are the result of decomposition? By no means; and for this reason. If the temperature of an organized substance be raised sufficiently high, it will not only destroy whatever micro-organisms may be present, but it will also destroy its molecular life, thus making it impossible for the disintegrating and transforming processes of vitalized chemistry to take place.

Again, bacteria are found more abundantly in localities where there is the greatest amount of decomposition. This Pasteur demonstrated in his controversy with Pouchet and

others while investigating the subject of heterogenesis. He found them "more thickly spread in towns than in the country, while the germs became fewer in proportion as they recede from human habitations. Mountains have fewer than plains, and at certain heights they are very rare." To test this, Pasteur took with him sealed tubes containing putrescible substances and opened them at certain points, with the following results: "Of the twenty bulbs first opened in the country, eight contained organized productions. Of the twenty opened on the heights of Jura, five only were altered, and of the twenty opened upon Montanvert during a strong wind blowing from the Mer de Glae, one alone was altered."

That organic matter in the process of decomposition should assume new forms is not without a precedent. Take for example the decomposing processes of chemistry; the solid rock is disintegrated, and even fused, yet none of its gases or bases are lost, but, becoming united with other acids or alkalies, form new compounds wholly unlike the original substance. Considerations like these have led me to the conclusion that *bacteria are the result* (as I have already said) *of the decomposition of organic substances.*

But, you may say that this is spontaneous generation, or life from inert matter. By no means. Life, as I understand it, exists in two forms—life *molecular* and life *spiritual*, *both* of which are God-given principles, *neither* of which are lost by the death of the organism. At death, life spiritual "returns to Him who gave it," while life molecular, in the transformation of the molecules of matter, continues to exist, although in connection with new forms. This is illustrated by an examination of certain forms of organic life, e. g.: Witness the process of nature in the construction of a plant. The soil, from which it receives its nourishment, is solid rock comminuted by the elements, heat, cold, air and moisture being the principal factors. From this solid rock so comminuted, that principle in the seed which is called life constructs the plant, the flower, the fruit, imparting to each molecule of inert matter its life. Again, take a leaf from a living plant, remove a portion of the epidermis and

place it under the microscope, and you will see millions of little cells placed side by side, each of which contains the unseen vivifying principle, the molecular life—the life of the plant. Now, when this plant undergoes the change which we call death, is its life principle lost? Not at all. It is merely transferred to other forms. That this life principle is transferred from one particle of matter to another is further illustrated, but in a higher sense, in the reproduction of organic bodies. In the flowering of plants, e. g.: the pollen grain, thrown off from the male organs of the flower, after penetrating the ovule, unites *with it*, and transfers its vivifying principle *to it* in such a way that a new organism is the result of such coalescence. So, in the process of decomposition, of which I have just spoken, this molecular life attaches itself to other particles, and reverts, so to speak, to the primordial condition of matter, viz: protoplasm inclosed in delicate cell walls. It is life under different forms—forms brought into existence in accordance with the reorganizing processes of Nature's laws; forms *essential* to the decomposition of organic matter. They are the wheels *within the wheels* of Nature. They are the unicellular organisms that I have already described—the bacteria—which, when once formed, live upon the medium with which they come in contact, and absorb its benign or malignant influences, as the case may be, and being wafted hence, they carry with them the poison upon which they have fed. This, I know, is not in accordance with the views of Koch, Cohn, Pasteur and many others, as well as our own Sternberg, who argues at considerable length to prove that bacteria *alone* are the cause of disease and death. In a work published by himself in 1884, Part V., under the heading, "Bacteria in Infectious Diseases," he says: "No more important question ever engaged the attention of physicians, of sanitarians or biologists, than that which relates to the rôle of the bacteria in infectious diseases. The practical results of etiological studies, so far as the prevention and cure of diseases are concerned, are likely to be much greater than those which have been gained by the pathologist; and if the time ever comes, as

now seems not impossible, when we can say, with confidence, infectious diseases are parasitic diseases, medicine will have established itself upon a scientific foundation."

After stating that this generalization would be "premature in the present state of science," he asserts in many places in his work, that it has been fully established in certain diseases; as for example, anthrax, fowl cholera, etc.

In addition to this, he made an unwritten statement on the floor of the San Francisco Microscopical Society just before he left for the East, in which he claimed that Koch had, without doubt, established it as a fact, that bacteria are the cause of tubercular phthsis.

The experiments, which in his opinion establish what he claims, he introduces as follows: "It is now generally admitted that the only satisfactory proof that a certain microorganism bears a casual relation to a disease with which it is associated, is that which is obtained by a series of culture experiments, in which the organism is completely isolated from the non-living constituents of the infective material containing it, and in the production of the disease in question by inoculation experiments with such a pure culture." The unimpeachable nature of this proof, when the experiment is properly made, and frequently repeated with the same result, is made apparent in the following quotation from a paper by the writer relating to a "fatal form of septicæmia in the rabbit." He then gives in detail his process for diluting the virus by carrying it through eight successive cultures, and then relates the following experiment: "Sept. 14th, injected ten minims of culture No. 8 into a full-grown rabbit. *Result*:—This animal died at 9 A.M., Sept. 15th, and a microscopical examination made at once demonstrated the presence of the micrococcus in great numbers in the blood and effused serum in the sub-cutaneous connective tissue." His conclusions are as follows: "This experiment shows that the micrococcus retained its vitality at the end of six weeks, and, very conclusively, that the virulence of the culture fluid is due to the presence of the micrococcus, and not to a hypothetical chemical virus found in the first instance in human saliva, and subsequently in the blood of a rabbit inoculated

with this fluid." For the purpose of showing that this "chemical virus" was "hypothetical," he submits the following calculation, viz: "My culture-tubes contain about a fluid-drachm of sterilized *bullion*. The amount of blood introduced into culture No. 1, as seed, was considerably less than a minim; but, for convenience, I will suppose that one minim was used each time to start a new culture, that is, the original material is diluted 60 times in the first culture, 3,600 times in the second, 216,000 times in the third, and in the eighth culture it will be present in the proportion of one part in 1,679,611,600,000,000. Yet a few minims of this eighth culture possesses all the virulence of the first." After making some remarks of a similar import, he sums up as follows, to wit: "When, in addition to this, we remember that potent chemical poisons, especially when injected subcutaneously, act promptly, and that their poisonous effect bears a relation to the dose in which they are administered; whereas a rabbit subjected to an experimental inoculation with septic blood, or with a culture-fluid remotely inoculated with this material, shows no signs of ill-health for many hours—eighteen hours or more—and that it is only when sufficient time has elapsed to permit of the abundant development of the micrococcus that serious symptoms are developed, we shall see that but one conclusion can be drawn as regards the rôle of the micrococcus." He further says, that "it is by experimental evidence of this nature that Koch, Pasteur, and many others have demonstrated beyond question that the disease known as anthrax is produced by a parasitic micro-organism—the bacillus anthracis." Again he says: "It has been suggested that the parasitic micro-organism in these diseases is, perhaps, only a secondary cause, being merely a carrier of the non-living ferment, which is the special poison of the disease. This hypothesis, also," he says, "is excluded by inoculation experiments with pure culture, sufficiently removed from the natural infective material. For the organisms introduced into culture No. 1, as seed, disappear as quickly from successive cultures as does the non-living material with which they are associated, and we may soon leave them out of the account,

although each successive culture-fluid is invaded throughout by their numerous progeny."

From the foregoing quotations it is apparent that the point relied upon by those who advocate the germ theory of disease, is contained in the statement that the organisms introduced as seed, as well as the non-living ferment, or poison, disappear in successive cultivations and thus leave the bacteria *alone* as the true cause of disease.

That this is erroneous will appear when we consider that the conclusions to which these scientists have arrived are not in harmony with some of the established facts of science, e. g.: It is a principle in natural philosophy, as old as the science itself, that matter cannot be so finely pulverized that it may not be again divided. This corresponds with our every day experiences in the use of poisonous substances as medicines. We find that divisibility *does not* destroy the medicinal power of a drug. Not only so, but the divisibility of matter frequently sets free a curative *force* that had previously been a sealed book.

Now, what is true of the potential poisons is doubly true of the chemico-vital poisons (ptomaines). Divisibility *does not* lessen *their* poisonous qualities, because they act like the ferments. Each particle touched by them, is as thoroughly poisoned as the first, in fact, it becomes poison itself, so that, it is of no consequence whether the poison passes through one culture-tube or five hundred; the *last particle touched is as thoroughly poisoned as the first in the series.* Another cause of error in the conclusions drawn from these experiments, consists inlosing sight of the true nature of the bacteria.

Now, it is obvious, that just in the proportion that vitality ceases in an organized body, fermentation commences, and its progress corresponds to the presence or absence of the conditions necessary to decomposition. So, that, wherever there is fermentation there is decomposition, and *vice versa.* It follows, therefore, so far as organized bodies are concerned, that fermentation and decomposition are synonomous terms. Furthermore, when we consider that fermentation occurs in the interior of organs having no connection with

the exterior, as readily as elsewhere, it is obvious that it is a principle implanted *in the nature* of all organisms, and, therefore, *not* dependent upon external influence. And since the processes of all organic bodies, in their life and growth, are carried on by cell formation, so also, in the process of decomposition, the ferments, in accordance with inherent law, assume the form of cells, each ferment being recognized by its peculiar form. Now it is to these forms, or ferments, that science has given the name bacteria.

That all the bacteria are ferments is evident from the culture experiments of Koch, Pasteur, Sternberg and many others. For, like the recognized ferments, whether placed in artificial culture tubes or the areolar tissue of the rabbit or guinea pig (which are only culture tubes on a larger scale), they multiply in the same way and under the same circumstances of temperature, etc., until their supply of pabulum is exhausted. Their behavior is in all respects like the fungi that are admitted to be ferments. The bacteria ARE FERMENTS. Hence, we have the bacteria in yeast fermentation—the acetic and the vinous not only, but the septicæmic, etc., also. As to whether these ferments are noxious or innoxious depends upon the *conditions* that *surround them;* or, as we have already said, *depends upon the substances on which they have fed.*

We have already called attention to the argument of Dr. Sternberg, which is, that by passing through eight of his culture tubes, the original poison (as well as bacteria) is lost in the multiplication of the progeny. This we have already shown to be an error. But, to amplify it somewhat, take as an example septicæmia, which, being a ferment, communicates its poison, as we have just demonstrated, to the first particle it touches of the medium into which it is introduced, and this particle poisons the next, and so on, multiplying *pari passu* with the increase of the fermenation.

As a practical illustration, let the point of the finest needle, which has been dipped in septicæmic blood, pierce the tip of one of the fingers. From this minute point the poison will spread from particle to particle, until the inflammation (which in this instance is simply fermentation in

the living tissues) has spread up the hand, up the arm, through the whole body. Now, we all know that every drop of blood in that body thus poisoned, —yes, even the decillionth part of a drop, contains sufficient poison to destroy the strongest man if introduced beneath the skin.

This idea is well illustrated in a series of experiments by Davaine, who in 1872 made a report to the French Academy of Medicine of his researches, in which he attempted to find the smallest quantity of putrid blood that would kill an animal. He commenced by injecting ten drops of putrid blood taken from the heart of an ox killed ten days previously, and very fetid, into the subcutaneous tissue of the neck of five rabbits, half of which died; and from the heart of one of these dead rabbits blood was taken and diluted to a degree of great minuteness and injected into the subcutaneous tissue of other rabbits, with deadly effect. Not contented with this, he continued his dilutions, the result of some of which I will give in his own words. He says: "In the following generations I reached quantities whose minuteness was beyond all expectation." Then after narrating some experiments, he says: "But, in spite of my knowledge of these facts, I could scarcely persuade myself that the death of the animals inoculated with the infinitely small doses to which I had arrived was not the effect of some error of my operations. I surrounded myself, therefore, with the most minute precautions, both in the calculation of the successive dilutions and as to the cleanness of my instruments and vessels, which I frequently bathed in alcohol. But very soon the consonance and invariableness of the results obtained proved to me that they were untouched by error." I will not take the time required to give in detail all of his experiments; two or three of the last will be sufficient. He continues: "In the twenty-second generation three rabbits were innoculated with a millionth, a hundred millionth and a billionth of a drop of the blood of a rabbit, dead *two hours* previously, which had been poisoned by a five hundred millionth of a drop of septicæmic blood. These three rabbits died, two in about thirty-six hours and one in forty hours. In the twenty-fourth generation, five

rabbits were inoculated from the blood of another dead from the hundred millionth of a drop. The first received a hundred millionth, the second a billionth, the third a ten billionth, the fourth a hundred billionth and the fifth a trillionth of a drop of the heart-blood of this animal. All were dead before twenty-four hours had expired."

Again, sceptic poison resembles the other ferments in this, as Davaine has shown, viz: it becomes inert if kept beyond a certain time. He says: "Ox blood, kept for ten days, was far more virulent than that preserved from eleven to sixteen days." And again, in speaking of septicæmic blood, he remarks: "On the ninth day after decease of its owner it was found still vigorous, but by the twenty-third day it had become innocuous."

I know that the advocates of the germ theory will claim that the innocuous quality of the septicæmic blood in the experiments of Davaine was due to the fact that the contained bacteria died. That they died is doubtless true. But it does not disprove the statement that their malignity was due to an evanescent chemico-vital poison. These chemico-vital poisons are especially malignant when they are the result of the putrefactive decomposition of animal tissues in a state of disease. Now, if we separate one of these poisons from the microbe with which it is associated and inject it into the subcutaneous tissue of an animal, not only will the same diseased condition be produced, but also the organisms peculiar to such disorganizations. That this is so, and that the bacteria so produced are in this sense the creatures of such a poison is shown by recent investigations. In 1884 "the discovery was made in the Pathological Institute of Professor Semmer, in Dorpat, that the bacilli and micrococci of anthrax are the *product* of a *septical anthrax-virus*. Rosenberg already had shown that by inoculation with boiled septic blood, *free* of *micro-organisms*, the symptom-complex of septicæmia was produced, and in the animals experimented on, after perishing, the *same micro-organisms* had been found as in animals which had perished after injection of unboiled septic blood. Animals inoculated with boiled anthrax-virus which perished in from

three to six days had the anthrax-bacilli in a quarter of the cases, and in *all* the first grades of development of the bacilli."

In speaking of this, Osal says: "This fact, by itself alone, even if *no* typical micro-organisms were found in the blood of the animals which perished in consequence of the injection of boiled anthrax-blood, would clearly indicate a *chemical anthrax-poison*. The bacilli do not represent the primary, but the secondary condition, and receive their virulence only after the influence of an *inorganic chemical poisonous substance*." This view is sustained by the experiments of others on anthrax-blood. "Paul Bert has been able to isolate a poison, diffusible in liquid, which kills in twelve hours. This he accomplished by destroying the bacillus in a fluid, containing it by means of compressed oxygen." Toussaint, also, obtained evidence of the presence of such a poison. Sternberg says: "We have experimental evidence that most potent poisons are produced during the putrefactive decomposition of organic matter. The poisons * * * called ptomaines by Selmi (who first obtained them from a cadaver), are fatal to animals in extremely minute doses." The experiments of Hiller and Kusner demonstrate the same thing.

Klebs, I know, claimed that after filtering anthrax blood he found that it had lost "its infectious properties;" but notwithstanding this, the testimony of the authorities just quoted, remains unimpeached. Furthermore, in an article entitled "Recent Investigations upon Cholera," published in the March number of the "Science," 1885, we find that Villiers speaks of an alkaloid (ptomaine) found in the cadavers of two persons dead of cholera. It was found in notable quantity in the intestines, and in much less quantity in the kidneys, liver and heart's blood. It is a liquid, has an acrid taste, and a distinct odor of hawthorn. It is alkaline, etc., etc. This article also states that "The English Cholera Commission has made a full report of its labors, which seems to contradict Koch's assertions in every vital point."

In an announcement made in 1880, Pasteur calls attention to some experiments made with attenuated or modified

virus. He claims "that the microbe of fowl-cholera can be modified, by special treatment, in such a manner that it will no longer produce a fatal form of the disease; and that fowls inoculated with it were subsequently protected *against* the disease, resisting inoculation with the most potent virus." This result he obtained by exposing it to "contact with the oxygen of the air." Toussaint proposed to accomplish the same result by the application of heat. This, according to Chauveau, "is best accomplished, in the bacillus anthrasis, by exposure for eighteen minutes to a temperature of 50° C."

Dr. Sternberg says, that "in these protective vaccinations, the virus used is either greatly diluted,* or is modified as regards the reproductive activity of the parasite by exposure to oxygen, by heat or by certain chemical re-agents." He further says, "this recovery, after inoculation with attenuated virus, is more easy to understand than is the subsequent protection."

Pasteur attempts to explain it by supposing that there is a want of pabulum in those cases that are protected, so that when inoculated with a virulent form of the virus, the germs will not multiply. Tyndall, in his introduction to the "Life of Pasteur" (republished in January, 1885), claims to have made "such an explanation of non-re-current diseases to an eminent London physician nearly fifteen years ago." Dr. Sternberg thinks that the explanation "is that the vital resistance offered by the cellular elements in the bodies of these two individuals was not the same for this poison."

Neither of these suppositions account for the protection that follows vaccination. We know, from experience, that the severity of variola is mitigated by inoculation with pure variola virus; but the disease produced is still the small-pox. The vaccine disease, however, is different yet protective. Now, it would be as reasonable to suppose that the mitigation which follows the inoculation with small-pox virus, is due to a lack of pabulum in the system, as to suppose that the want of it after vaccination is what affords protection. The *true* explanation is undoubtedly in accordance with

* Davaine's experiments disprove this.

the *law of similars*. All agree that the virus is changed by exposure to oxygen, heat, etc., etc.; doubtless so changed, that it is not the *same* chemico-vital poison; still, it is so *similar* that the difference cannot be detected by the highest magnifying powers; and because of this *similarity*, protection follows its use.

The point, however, which I wish to bring out here, is that the difference between the "most potent virus" and the protective or modified virus, *does not consist in a change in the bacteria*. This is well illustrated in the case of variola; and right here let me say, that it is admitted by all who have examined the subject, "*that bacteria are not found in all those who suffer from variola.*" But *when* found, they are identical in all their morphological characteristics with those found in pure vaccine virus. The language of Cohn, Hallier, Weigert and others on this point, is that they "*find in vaccine lymph micrococci analogous, in every point of view, to those of variola.*"

The same is true of the other diseases mentioned. None of the experimenters claim that there is any change in the microbe. Here then we have an anomaly. Two individuals are inoculated, the one with the most potent virus, the other with the modified virus, both of which contain microbe that are in *all* respects *identical*. Now, it follows, that if it be the bacteria that are the cause of the symptoms that are developed, the result ought to be the same in both cases; but behold the contrast! In the one we have a loathsome disease; whereas, in the other, the system is not only touched gently, but is forever afterwards protected from an attack of the more virulent disease. *Wonderful discovery! All honor* to *him* who first discovered it.

Since the above was written my attention has been called to an article by Rollin R. Gregg, M.D., entitled "The Science of Fibrin," which was published in the November and December numbers of the "Medical Advance" for 1883. I have carefully read the statements made by Dr. Gregg in those papers, as well as several other articles pro and con, published in subsequent numbers of the same journal. His observations are quite remarkable. He took fresh blood, and after boil-

ing it, put it into a bottle, corked it and watched the results. After a while he saw, as he says, "all the forms that the bacterists ever pictured or saw in their bacteria;" that they "are exactly repeated in boiled blood, and there are many more forms found therein than they have ever mentioned. There are the so-called micrococci by the million, the spherical bacteria, the "rods, the spirals," etc. He says he "has seen them taking on a lower or vegetable life under decay," which, he says, "brings up the whole subject of cultivation." etc. He also asserts that in undergoing these changes "the fibrin appeared to eat up or attract to itself all the rest of the contents of the bottle, the blood corpuscles, fatty matters, albumen, salts, etc., and thereby had the food supplied to enable it to develop all the great varieties of forms that it went through." In short, he claims that fibrin performs the functions of absorption, assimilation, reproduction, and, in fact, all those functions which we know are performed by the unicellular algæ and other microscopical plants. Now, it has long since been demonstrated that where two things equal a third, they are equal to each other.

From the quotations made from Dr. Gregg's observations, as well as everything that he has written on the subject, it is clear that his fibrin, as we have just remarked, behaves in all respects like the unicellular algæ. And according to the observations of a long list of very able scientists, the microscopical bodies that are found in such profusion almost everywhere—the bacteria—also behave in all respects like the unicellular algæ. Now, since Dr. Gregg's fibrin and the bacteria are both equal to the algæ, they must be equal to each other; they are both microscopical plants; they are both bacteria.

If the observations of Dr. Gregg be correct, he has furnished us with the most positive proof of a proposition assumed in this paper, viz: that bacteria, wherever found, are the result of the decomposition of organized matter. It is clear, therefore, that the remarks of Dr. Gregg do not answer the question: Are bacteria the cause of disease? As we have already said, the advocates of the germ theory

think that because a drop of septic blood can be passed through a series of culture tubes, and because a few minims from the last culture tube, when injected into the areolar tissue of an animal will be followed by septicæmia, and because the bacilli found after death are the same kind of bacilli as those in the original septic blood, that therefore they have settled this question in the affirmative.

Now, it cannot be successfully denied that when a fluid which contains bacteria that have absorbed a chemico-vital poison (no difference though that poison be septicæmia, syphillis or vaccine), is injected into the skin of man or animal, so as to be taken up by the *absorbents*, it will follow the law peculiar to that poison—it will reproduce itself. This is done in accordance with that universal law of nature—"each after his kind"—which is as true here as in the procreation of species. This only proves what we have always known and have already said, viz: that a ferment introduced into a suitable medium will communicate its properties to the entire mass with a rapidity proportionate to the conditions present. But *this* does not prove that bacteria are the cause of disease.

To prove that one thing is the cause of another, it must be shown that the cause was in active presence before the thing produced was manifest. In the case of bacteria, therefore, it must be shown that they are present in the blood of a given case before the disease manifests itself; for, if they be not present until *after* the disease has brought the system under its influence, the inference is that the bacteria in that case, instead of being the *cause* are the *result* of the disease. Any other conclusion would be illogical.

Now, it is a fact that bacteria are *never found in healthy blood*. This is not only admitted, but it is most emphatically asserted by some of the advocates of the germ theory. On this point Koch remarks: "I have on many occasions examined normal blood and normal tissues by means which prevent the possibility of overlooking bacteria or of confounding them with granular masses of equal size, and I never, in a single instance, found organisms. *I have, therefore, come to the conclusion that bacteria do not occur in the*

blood, nor in the tissues of the healthy living body, either of man or of the lower animals." Not only so, but Sternberg says: "If we inject a small quantity of a culture-fluid containing the bacteria of putrefaction into the circulation of a living animal, not only does no increase and no putrefactive change occur, but the bacteria introduced quickly disappear, and at the end of an hour or two the most careful microscopical examination will not reveal a *single bacterium.*" Not only so, but when taken into the healthy stomach they are harmless. This is shown by the fact that the comma-bacillus, which Koch claims to be the cause of Asiatic cholera, can be taken into the stomach by the *million* without unpleasant results. Thus Hallier remarks, "that in epidemics the cholera-fungi pass through the healthy intestinal canal, *en masse*, without injury." Dr. Klein swallowed "the living comma-shaped microbe" without bad effects. Koch himself, after numerous failures to produce cholera in the lower animals, had to inject the bacilli through the coats of the bowels into the duodenum. He claims that "with few exceptions the animals so treated died within a space of time extending from a day and a half to three days." In speaking of these experiments in a letter published in the February number of the "Science" for 1885, Dr. Sternberg, although a strong believer in the germ theory of disease, very properly remarks: "We are disposed to receive the proof now offered with some reserve, inasmuch as the injections seem to have been made through the walls of the abdomen directly into the intestines. This method has no doubt been adopted upon the supposition that previous failures were due to destruction of the bacilli by the acid juices of the stomach. There is nothing improbable in this supposition; but, on the other hand, the possibility that when the material is injected into the intestines the puncture made may have been a serious complication and source of error." But, I say, even if it were *not* "a serious complication and source of error," the finding of them, or *any* particular form of bacteria in the excreta, or even in the *blood* of a patient sick with cholera, malarial fever, yellow fever, or *any other* form of disease, is not sufficient evidence,

as I have already said, to entitle them to be considered the *cause* of disease.

To establish such a proposition in any given case of disease, as for example, Asiatic cholera, it ought to be shown that the comma bacilli are, first of all, found in the jungles of Asia, or the "bacillus malariæ" ought to be found in the swamps of Florida or Michigan; or a yet unnamed yellow fever microbe should be easily discovered in the marshes about New Orleans or Memphis during an epidemic; and these, again, should not only be found in the blood previous to the commencement of the given disease, but they should always be present in *every such case of disease*. When all *this* is done, *then*, and *then only*, will we have the kind of evidence necessary to establish the germ theory on a scientific basis. But unfortunately for the theory, the very opposite of this is true.

In the "Preliminary Report of the Havana Yellow Fever Commission of the National Board of Health," we have the following: "In Havana, Dr. Sternberg gave a large share of his time to the microscopic examination and photography of the blood. * * * The patients from whom specimens of blood were obtained were mostly soldiers in the military hospital of San Ambrosio. Ninety-eight specimens from forty-one undoubted cases of yellow fever were carefully studied, and one hundred and five photographic negatives made, which show satisfactorily everything demonstrable by the microscope." After stating that "these photographs were mostly made with a magnifying power of 1,450 diameters, obtained by the use of Zeiss' one-eighteenth objective and Tolle's amplifier," * * * the report says: "If there is any organism in the blood of yellow fever demonstrable by the highest powers of the microscope as at present perfected, the photo-micrographs taken in Havana should show it. *No such organism is shown* in any preparation photographed immediately after collection."

So, also, in regard to the "bacillus malariæ." Dr. Sternberg, after long and patient search in the "swamp-mud near New Orleans, and in the gutters within the city limits," and elsewhere, exclaims, "who on this side of the Atlantic

has seen the bacillus malariæ?" Now, although such a discovery has been claimed by some scientists in Italy, still, the fact remains that there is not sufficient evidence anywhere extant, to establish the discovery of such an organism.

In discussing the subject of malaria, Dr. Sternberg says, "We may admit the possibility that its action (the 'hypothetical parasite,' as he calls it) is restricted to the production of a volatile chemical poison, which is evolved as a result of its vital activity in the localities where it abounds external to the body; and that this (volatile chemical poison) infects the atmosphere in the vicinity and produces malarial poisoning in those who respire this atmosphere."

This comes very near the truth, for there is no evidence to show that malaria is anything other than a "volatile chemical poison," evolved in the form of gas as one of the products of the decomposition of vegetable matter, which, being modified in its effects by different constitutions, temperature, habits, etc., is the probable cause of the various forms of malarial diseases. But it is only when the *vitality* of an organism is depressed that these vitiating influences cause any disturbance in the system. This is admitted, when the advocates of the germ theory claim that the conditions must be favorable to the propagation of bacteria, or a given disease will not be produced. For example, they claim that the tubercle bacillus is the cause of consumption. But, they say, "the conditions must be favorable." Now we all know, that there is a physical conformation which, in the majority of cases, is a constant concomitant of tubercular phthsis. So true is this, that the practiced eye can frequently predict the result of a case years before any active disease is manifest. Are the tubercle bacilli, then, the cause of the death of so many millions, whose hereditary tendencies can be traced backward from generation to generation? Are they the cause of the tubercles in the brain of the child yet unborn? Are the "crypta syphilitica" the cause of the *caries*, the *necroses*, and the thousand and one other maladies that follow in the train of a disease contracted by an erring progenitor?

The absurdity of such a proposition is self-evident.

Again, Neisser claims that he has discovered a micro-organism in the pus of gonorrhœa, and tries to show that they are the cause of the "specific virulence of the fluid in which they are found." Weiss also has found these organisms in the "pus from the male urethra, and in that from the female vagina,—in blenorrhœ neonatorum, and in gonorrhœal ophthalmia." They also claim "that they are not present in the secretion of simple urethritis."

The observations of Mr. A. S. Keyser (of the University of Maryland) confirm the statements made by "Neisser as to the constant presence of the 'gonococcus' in specific purulent discharges."

"Neisser claims that this micrococcus has distinct morphological characteristics." But his observations are not sustained by other microscopists. Dr. Sternberg, for example, after much painstaking and very persevering investigations, says that his "observations have *not led him* to the conclusion that these morphological characteristics are peculiar to the micrococcus of gonorrhœal pus." He has found a micrococcus "in normal human saliva," which resembles it in all its morphological characteristics. Numerous other observers have found similar organisms in "acute abcesses from surgical injuries, etc." Ogsten has given much attention to the study of these, and in his report on "Micro-organisms in Surgical Diseases," he gives figures of micrococci which resemble very closely, if they are not identical with the "gonococci of Neisser."* To settle the question of morphological characteristics, Dr. Sternberg made numerous culture experiments with "gonorrhœal pus taken at the moment of its escape from the *meatus urinarius*," and the "pus from a deep seated abcess." "No difference was detected in the morphological characters, or in their behavior in a culture-fluid, between the micrococci from these two sources."

The conclusion drawn from this is that the only difference between the micrococci found in "normal human saliva" and those found in "deep abcesses," as well as the "gonococci," consists in the *media* in which they are

* Sternberg.

found. That the so-called "gonococci" are found in gonorrhœal pus, and that when brought in contact with other mucous surfaces, a similar inflammation will be excited, is not to be denied; but that they are the *cause* of the "specific virulence of the fluid" in which they are found is *most emphatically denied.*

What, then, is the cause of gonorrhœa? Having had quite extensive opportunities for observation in this disease, I am led to conclude that it is, in the first instance, an inflammatory condition of the mucous surface of the vagina, caused, it may be, by cold, want of cleanliness, excessive sexual intercourse in constitutions rendered susceptible by scrofula or other hereditary tendencies. That as a result of this abnormal condition, a chemico-vital poison is secreted in connection with a more or less abundant flow of mucous, thus constituting an acrid leucorrhœa. Now, as I have said, there are micrococci in all the orifices of the healthy human body, and being already present in the vagina when this poisonous condition manifests itself, they live and multiply *in* it and absorb *from* it the so-called "specific virus," which is capable of producing similar inflammations when brought in contact with other mucous surfaces. To repeat, their rôle in this disease is clearly this: Being present when the parts are in a normal condition, they are harmless, but their innocuous character is immediately changed as a consequence of the pathological condition of the parts. In other words, from being a harmless ferment, they are changed into one of a poisonous nature, and thus become, as I have said, *carriers* of *poison.* These remarks apply with equal force to all the septic poisons. None of them are productive of harm unless *introduced* into the *absorbents* by *inoculation,* or by *direct contact with an absorbing surface.*

These remarks, however, do *not apply* to paludal fevers, or to those diseases which are called zymotic. Those who believe in the germ theory have labored long and hard to show that bacteria are the cause of such diseases, but so far they have utterly failed to establish any such relationship.

Again, the germ theory has suggested a new line of rem-

edies, viz: germicides, which, if the theory were correct, would be rational remedies, but unfortunately for the theory, the practice has been a failure. It is now admitted that in order to destroy the germs in a given disease, the necessary amount of the germicide would endanger the life of the patient.

On the other hand, the treatment of zymotic and infectious diseases scores another point against the germ theory, for hundreds upon hundreds of cases might be cited to show that the most malignant types of these diseases have been cured by remedies selected in accordance with the law of similars. Remedies, too, which in themselves are not germicides, the cures being frequently effected by the *higher*, and in many cases the *very highest potencies*.

The inference from all this is *irresistible*, that bacteria are not the cause of disease.

It is equally obvious that their presence as a means of diagnosis, in certain cases, is of great value.

Resumé.

The bacteria are widely diffused, unicellular, vegetable organisms that can only be seen with the microscope.

The bacteria are the result of the decomposition of organic substances in accordance with inherent law; they are ferments.

The appearance of bacteria is sometimes changed by the *pabulum upon which they feed.*

Septic, and other infectious poisons, can be modified by various re-agents (without changing, in the slightest degree, the forms of the contained bacteria), so that when introduced into the system by vaccination, they will protect the individual against the more potent virus.

The theory that bacteria are the cause of infectious diseases is false, because their presence is not necessary to produce the diseases ascribed to them. They are only *carriers* of *poisons* (ptomaines), which are generated during the *decomposition of organic matter.*

The bacteria *cannot exist in healthy organisms.*

To demonstrate that bacteria are the cause of certain diseases, it must be shown that their original habitat is external to the body, and also, that they are present in the blood and tissues before the disease manifests itself, and that they are *always* present in *every such disease;* the *reverse* of which *is true.*

The theory, that the use of germicides in infective and zymotic diseases, is scientific treatment, has been exploded by practical tests. Such treatment would jeopardize the patient.

It has been demonstrated by hundreds of practical tests, that paludal fevers, infectious, as well as zymotic diseases, have been cured by remedies chosen in accordance with the *law* of *similars,* many of which remedies are not germicides. These remedies, too, have been successfully used, not only in the *higher,* but in many instances, in the *very highest potencies.*